

Writing as a responsibility of science: a reply to Laband and Taylor.

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It must have been around 1968, in the coffee room of the Social Science Building at Chicago, that Milton Friedman and the late George Stigler had a jovial and public conversation about being economists that made a big impression on me. Milton was lamenting the stupidity of tariffs, and George broke in, from a foot above, saying something as follows: "Milton, you're such a preacher! If people want free trade they'll get it. If they don't want it, no amount of jawboning by economists will change their minds." "Ah: that's where we differ, George. We admire markets, but you think they've already worked." "And why not? People are self-interested, and they vote their pocketbooks -- that's enough to make the market work. They bought tariffs; tariffs must be what they want." "No: they pursue their interests but often do not know what their interests are. People need education. The average citizen has no idea that a tariff hurts him." "Education! Try educating a lobbyist for the textile industry." "As I said, that's where we differ: I'm a teacher, and think that people do some things because they are ignorant." "And I'm a scientist, an economic scientist; people do what they do because they are wise."

Laband and Taylor are Stiglerites. They believe with Dr. Pangloss that we are in the best of all possible worlds. The Friedmanites, which is my own tribe, believe that we could be in it if we would only stop to think. The Stiglerites assume rationality; the Friedmanites teach it. The Stiglerites want to praise the world; the Friedmanites to change it. The Stiglerites detest policy; what is, is. The Friedmanites embrace it; what might be, can be. The Stiglerites are pessimistic, in the manner of the master. The Friedmanites are optimistic.

Laband and Taylor, the reader can see, have a pessimistic and Stiglerite explanation of why they have not learned to write competently. They defend themselves against advice such as I give in the original paper by saying that it is "presumptuous of anyone to claim unique knowledge of the marginal benefit to authors of investment in better presentation."

If the investment we were talking about was oil drilling, I'd agree with their economics. I've written a book called *If You're So Smart* that makes a similar point about economic experts going around offering advice. But the investment in question -- here is where Laband and Taylor go wrong -- is economic education. Laband, Taylor, and I are economic educators. In the educational industry we are the oil drillers, the experts. We educate our student

roughnecks in the craft, teaching them because we do know better (or else we should not be standing up there TuTh 10:50-12:10). Part of the craft, Laband and Taylor agree, is "attending to elements of presentation."

So the initial mistake in their paper is economic. They think that because some advice is silly and misinformed, all advice must be, even advice from teachers to students. Stiglerites tend to drive themselves into such extreme skepticism. But no one can actually live as an extreme skeptic. I'd like to see what Laband and Taylor say to a student who thinks that markets are bad for you -- do they agree mildly that it's just a matter of opinion?

Another Stigler story. George opposed requirements in the graduate curriculum at Chicago. Around 1970 he killed off the requirement in the history of thought, which was his own field, and tried repeatedly to kill off economic history (he succeeded in 1980 when I left the faculty). His argument, which is heard in many other departments of economics, was that the graduate students should be free to choose (listen up, Milton). After all, they are just like consumers.

What George, and Laband and Taylor, never answered was the Teacher's Point, which Milton made in the discussions about the history of thought requirement. We are teachers. We do know better. Graduate students, because they are not educated already (that's why they are students), often make bad decisions about their curricula. (If Laband and Taylor looked back over the decisions they made as graduate students they would probably agree; I certainly am appalled at my own curricular decisions as a graduate student, such as spurning a course by the great Simon Kuznets because it was "just data collecting.")

To put it economically, the product at Chicago was the Ph.D student. If the student took a pattern of courses that left him an ill-educated dope, then all other Chicago Ph.D.s would suffer from his reputation. The faculty, not the student, had to decide on the qualities of the product. To do otherwise would have been to shirk entrepreneurial and managerial -- and pedagogic and scientific -- responsibility.

My paper in *Economic Inquiry* (and the little book of 1986 published by Macmillan, available, cheap, assign it to your students) was 98 percent teaching, Friedmanite. I was trying to change the taste of the producers and consumers of economic writing. That's called teaching. More than that, I was trying to change their moral attitude towards writing. That's teaching, too. I said so in my response to Jack High, which Laband and Taylor apparently did not read.

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Had they read the response they would know that they have misidentified my theme. They focus all their attention on the 2 percent of the paper that claimed that good writing is selfishly good for you (only part of the 2 percent, by the way, was the careerist argument that Laband and Taylor focus on; the rest was the argument of Socrates and other teachers that being good is -- selfishly -- good for your soul). Laband and Taylor think my "general theme" was that bad writing hurts the pocketbook.

But if I were making chiefly such a Stiglerite point in a Stiglerite world, what would be the point of writing the article? Economists do not have to be warned off using out-of-date statistical methods or studying topics that are not currently fashionable or failing to use constrained-maximization arguments as much as possible, any more than citizens need to be warned off protective tariffs if they do in fact vote their pocketbooks in selfish wisdom. The rewards and punishments in such a world are plain. Laband and Taylor's contribution is to note that the rewards and punishments to good writing are plain, too, under the assumption that the world is Stiglerite.

Their Stiglerite economics explains too much. If it were correct then we would now be in the best of all possible worlds with respect to the uses of econometrics, say, or the standing of this or that piece of economic theory. The producers of economic scholarship would already have adjusted to the demands of economic readers. By the Laband and Taylor argument there would be no economic argument, no place to teach people to do better, as Laband and Taylor might themselves want to be taught to do better in, say, the use of statistical significance or specification searches or entry and exit arguments.

Arguments about What Is To Be Done are the guts of science. Economists of taste -- for instance, Zvi Griliches, Kenneth Arrow, Gordon Tullock, Robert Solow, Robert Lucas, James Buchanan -- argue that economists should write well. Or they argue that economists should do more empirical work. Or they argue that economists should examine the quality of their data more thoroughly; or that they should be consistent in their assumptions about rationality; or that they should be literate in the history of their craft; or that they should not use statistical significance as the equivalent of economic significance (on the last point in full, for examples of such teaching, see Boring !1919^; Neyman and Pearson !1933, 296^; Kruskal !1978^; Tullock !1959^; Arrow !1959^; Morrison and Henkel !1970^; Mayer !1975^; Feige !1975^; Griliches !1976^; Lovell !1983^; Leamer !1983^; McCloskey !1985^; Denton !1988^). And having taught such general arguments they teach that economists should believe X or disbelieve Y about the economy. They do not suppose that on all these margins the optimal adjustments have always

already been made.

Close down the graduate schools, Laband and Taylor imply. Fire the editors of journals. Dismiss the referees. Let the consumers decide in their ignorance what they want to consume. Quality decisions are unnecessary in a capitalist paradise. Put every quality on the market -- economists who do know the crafts of economics, for example, and economists who do not, Chicago Ph.D.s who know there was a past to economics and those who do not -- and see what people buy.

I dunno. As an anarchist I admit that their vision (they do not know they have it; that's the trouble with not knowing the history of the craft) is not wholly unattractive. But Milton and I reckon we are at present in a second-best world, in which people need some education in responsibility before they are ready for the Second Coming of the market. We disagree with George, who came to believe that the Second Coming had already come.

My main purpose was not to establish the actual marginal product or the perceived marginal product of better writing. I know my colleagues do not put a high value on writing well; after all, that was why I felt moved to teach them, as one might try to teach them to use fixed point theorems correctly or to recognize that the Federal Reserve is speaking to an audience. I wanted and still want to change my colleagues' minds -- not by appeals to self-interest, chiefly, but by appeals to professional responsibility. Laband and Taylor do not want to argue about professional responsibility. They want to argue about profit.

They make a distinction between "personal" and "professional" morality in writing. Apparently as people they would like to work on their writing but, heh, they've got careers to run. They end up asserting boldly that they and other economists are and should be governed by "profit, broadly speaking." They give an example towards the end of the paper about the decision at the margin to flash some pointless econometrics or spend time on the writing to gain "adherents" (the point is made earlier, too, four times). They ask, "Is the economist a fool or irresponsible for not making such a substitution?"

Why of course he is a fool and irresponsible. He is unethical not to work on the prose, as he would be unethical not to work on the quality of the data or on the correct use of statistical tests. He is uncraftsmanly, unscholarly, unscientific. Comparative advantage, naturally, should be consulted; who said it should not? But everyone should try out every margin; economists do not know about the elements of style and therefore have no idea what better science they can do by taking care in

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writing. (For example, most economists, and Laband and Taylor, though they quote Salant on the matter, do not know that phrases like "knowledge claim production function" are unclear.) It would be as though economists did not know some obviously relevant tool such as statistical decision theory (actually, they do not). That was my point, the usual one in scientific discussions: do better. Not perfect but better. Science is difficult to do right. Forget about whether it is profitable, broadly or narrowly speaking. Standards of scientific responsibility constrain profitmaking. It may be profitable to fake your data but it is irresponsible. It may be profitable to write with a trowel but it is irresponsible.

To put the point another way, Laband and Taylor have a yuppie's idea of scientific responsibility. If the "market" demands it, pornography or not, print it. If one's career can be advanced, do it. Defect. This is why economics departments are hard to administer. About a quarter of a department, I estimate, think that being an economist means never having to think about responsibility. They think that the right behavior in the face of a group responsibility is to defect from it. (A classic experiment found that most people cooperated in prisoner's dilemma games more than optimally, being properly socialized. Which do you suppose was the only group tested that followed the economist's model of irresponsible defection?)

So a reply in Stiglerite fashion to Laband and Taylor would be lacking in point. The point of my article was what should be done in a fully responsible economic science, not the testing of the profitability of what is actually, irresponsibly done.

But as I said I'm a Friedmanite optimist and believe in the efficacy of teaching, so let's teach. Price theory first.

Their price theory says that "the market" is superior to "a biased observer's criteria" of stylistic merit. Not in most price theory. The set of prices thrown up by a market are no more "biased" than a single observer's valuations, as the theory of index numbers tells us.

Their price theory justifies the use of computer programs on style by noting that "since the programs are marketed to would-be writers, the presumption would have to be that there has been some attempt to capture relevant aspects of style in these measurement techniques. Otherwise, it seems unlikely that they would survive and flourish in the marketplace." Not in most price theory. The demands of would-be writers, who by assumption do not know what they are doing, are not enough to assure that the company captures relevant aspects of style. Laband and Taylor would on the same basis recommend that you listen to the

late-night pitch for making a million on property deals, which must somehow capture relevant aspects of buying low and selling high. It has survived and flourished. That there is a sucker -- or a first-year graduate student-born every minute does not figure in their price theory.

Their price theory explains that the crummy style in economics has developed "to (presumably) communicate efficiently." Not in most price theory. Mind games and cheating, the use of obscurity to claim profundity, the appeal to fashion and status, the fear of straight talk, or simple and correctable ignorance of the elements of style can all explain bad writing; in a word, defection. One wonders what scientific world Laband and Taylor inhabit and whether they have heard of the prisoner's dilemma.

They claim towards the end of the paper that "the evidence suggests that economists communicate efficiently." Their evidence says nothing of the kind; the evidence of one's senses points the other way. I wonder if Laband and Taylor have ever sweated through a paper for an hour or two, discovering at length the five-cent point buried under the five-dollar words. I wonder if the average economist regards the style of the American Economic Review as an aid to her understanding of the science. The daily waste of our scientific time is not "efficient."

I am especially surprised that Laband and Taylor think that more math always makes for more "cost-effective forms of communication." People will say such a thing, because they want math-department values to win over engineering- and physics-department values (McCloskey !1991^), but no one actually believes it, except the small group of people who can read mathematics faster than English. Many professional mathematicians don't like symbolic bullshit, and say so. The set theorist Paul Halmos complains that "The author had to code his thought in !symbols^ (I deny that anybody thinks in !such^ terms), and the reader has to decode" !1973, 38^. Stanislaw Ulam, complaining along with many other mathematicians about the raising of the symbolic ante in the style of Nicholas Bourbaki, writes: "I am turned off when I see only formulas and symbols, and little text. It is too laborious for me to look at such pages, not knowing what to concentrate on" !1976, 275f^. Laband and Taylor like market arguments. Well, so do I. Here's an issue that actually can be settled by one; ask the members of the American Economic Association if they agree with Halmos and Ulam or with Laband and Taylor.

The "theory" in the Laband and Taylor paper, then, is feeble, a rewriting of the marginal conditions copied out from elementary textbooks. I'm afraid their "empirical work" is at the same level, firmly in the lower tail of modern economics. Let's teach.

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What's mainly wrong in the use of econometrics in the profession is that it is not used for serious inquiry but for reaffirming what everyone, especially the authors, already know (Cooley and Leroy !1981^). Laband and Taylor, for example, labor with their econometric machinery to deliver up the mouse of a result that the Wall Street Journal is better written than Journal of Political Economy. "Economists' writing varies according to the intended audience." Yes.

They use a crude instrument to detect good and bad writing. They pick a few indicators that will be easiest to quantify, and then make a proxy argument: "Writers displaying excellence on one margin of clarity (average words per sentence) would exhibit excellence on other margins of clarity." Well, maybe, and maybe not. I don't know in what metric the marginal argument could be tested. No one will be surprised that according to their results their instrument doesn't seem to measure anything: that's their main finding, that the instrument fails.

The one test of validity they offer suggests that there is something wrong with the instrument. They run the instrument on journals of English (I could have supplied them with a better choice of journals). The English professors do no better. Something is screwy. What conclusion do they draw? That the discriminatory power of the instrument is low? Back to the drawing board? By no means. They conclude that English professors write no worse than economists.

The one econometric mistake they do confess to openly and fully is omitted variables, so it would be boorish of me to emphasize the point. But, briefly, what is learned from the statistical "failure" of a misspecified model? We can do our econometrics more intelligently than this. With omitted variables the coefficients of the included ones are not even consistent. (It's hard to tell from their ill-written table, but it appears, by the way, that most of the style variables they pick from my list have the correct sign; I suspect I am reading it correctly, because Laband and Taylor would have said so if the signs were wrong; sign-testing is part of the routine of bottom-tail econometrics).

I was not providing material for a mechanical test of the profit to be had from being scientifically responsible. I was trying, as Milton did, to teach people who want to write better, out of a sense of scientific responsibility. But if for some reason you wanted to perform the test that Laband and Taylor want to perform you would want data that were valid on the face. One would think the correct way of collecting the data would have occurred to Laband and Taylor. The correct way is to put unidentified examples of prose in front of (1) expert judges of writing and (2) average readers of professional articles and then ask them

to grade the writing, with perhaps a little quiz on reading comprehension appended. That's what one does implicitly in concluding that Robert Solow writes better than, say, Donald McCloskey. And it is the procedure in competent studies of readability. Using sentence length because it is easy to calculate is not much of an argument for collecting the wrong data, though usual in economics; look for the keys under the lamppost because the light is better there.

Towards the end of their paper it becomes clear that Laband and Taylor mix up "effectiveness" and "clarity." Effectiveness means getting your way; clarity means trying to make it easier on your readers. I have no doubt that the most cited economists are "effective," by definition. But Hitler was effective. Nixon was effective. Lying is effective. Being pointlessly obscure is effective. Repeating marginal arguments copied out of elementary books over and over again is effective, at least in some circles. Claiming the character of a Technical Thinker by misusing statistical significance is effective, among economists who do not understand statistical theory.

The moral problem at the core of the Laband and Taylor paper is that they cannot imagine that being "effective," regardless, is not a complete guide to scientific responsibility in an imperfect world. They are utilitarians, as many economists are, which in the simple form Laband and Taylor espouse is the religion of barbarians, the enemies of good science.

In my response to Jack High I called his paper "friendly fire," that is, shells from one's own side that fall on one's own troops. Laband and Taylor's paper is unfriendly fire, sure enough, not from my side. Fortunately, though, most of the shells drop on the gunners who shot them.

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